

I

SOCIETIES IN SPACE:
THE LANDSCAPE
OF POWER

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Power – the ability to exercise control or command over others – is one of the most fundamental social realities. The various devices used either to restrict power or to institutionalise and perpetuate it are among the most characteristic features of human societies. This is an obvious area of interest for the social archaeologist.

Power as such is not often directly reflected in the archaeological record. Wealth finds more obvious expression, for instance in the discovery of accumulations of special, and by inference valuable, objects. Status is often reflected and maintained by the use of conspicuous symbols. Power, on the other hand, must be exercised to be visible: perhaps the most obvious expression of the sustained exercise of power is seen in the orderly structures of organisation. These abstract points merit more careful consideration than they have so far received from the archaeologist. For the reasons discussed in Chapter 1 there is at present little literature in the field of social anthropology on the material correlates of power such as might be useful to the archaeologist.

The archaeological record does contain vast amounts of data which are of potential value in understanding and reconstructing the exercise of power in past societies (see Trigger 1974). The most obvious indicators are symbolic: the use of symbols as 'instruments of domination' (Bourdieu 1977) is widespread. It has already been utilised by archaeologists concerned with systems of dominance, and Marcus (1974) has written in this context of 'the iconography of power', although, as indicated above, this is not easily distinguished from the iconography of status. Such an approach is only workable in practice, however, in cases where such a symbolic system is sufficiently well developed and explicit as to be potentially intelligible to the modern observer. In general highly specialised symbolic systems are seen only in well-established state societies, and the iconography of power in less centralised communities is not so readily analysed.

The approach followed here is a very much more general one,

resting upon the sound principle that, whatever the limitations of the archaeological record, most archaeological finds have a precise provenance in space. We know where they were found. It is a central idea of the two papers which follow that power has spatial correlates. For it is the essence of power relationships that they are asymmetrical: when all relationships are symmetrical, we are dealing instead with an egalitarian society characterised by what Durkheim called 'mechanical solidarity'. Symmetry and asymmetries associated with power can be understood, at least in part, in spatial terms.

There are other reasons than this for considering the spatial behaviour of societies. Indeed the archaeologist's first problem – and one which rarely delays the social anthropologist for long – is to define what he means, in a particular case, by a society. What is the *unit* of study? Such a question is by no means easy to answer, and the decision on the scale of the unit under consideration is one of the most crucial to the whole outcome of the research.

This matter of scale and the selection of the social groups with which the archaeologist deals has proved one of the most difficult problems in the practice of archaeology. In the first of the papers which follow it is argued that the decision made by Gordon Childe to define the archaeological 'culture' as the appropriate unit for the discussion of social questions was largely responsible for the subsequent disappearance of effective discussion of social questions for more than four decades. The same accusation can be levelled at the taxonomic systems developed in the same period in the United States. The basic misconceptions were to confuse questions of social organisation with those about ethnicity (for Childe and his contemporaries interpreted the term 'culture' in an ethnic sense), and to assume that arbitrarily defined taxonomic units (i.e. 'cultures') had clear ethnic correlates. As I seek to show below, both of these are errors of method. Different societies formulate their ethnic concepts in different ways and at different scales. The feeling of belonging to a definite group, of being one of a specific 'people' is indeed a widespread one in human societies, but it is too complicated a matter for ready assumptions.

The exercise of power in society implies asymmetry, then, and it is, moreover, the kind of asymmetry which results principally from centrality. Power is often, although not inevitably, concentrated into the hands of a restricted group of people, the investigation of the archaeological indications of such *central persons* is one of the most fruitful avenues for the elucidation of past power relationships.

One approach to this problem, and it is the one carried forward by a specific example in Chapter 3, is to assume a correlation between *central persons* and *central places*. The study of the location and exercise of central functions, and especially those of government, can then offer one of the most useful sources of information not only about the organisation of society but about the exercise of power in it, and about the scale of the units with which we are dealing.

At first sight this may seem a geographical question, as indeed in a sense it is. But the main thrust of geographical analysis has been in a different direction, resting upon the assumptions of Central Place Theory, about which there is a vast literature. Most investigations operating within a Central Place Theory framework have operated *within* a single known political unit, usually a modern nation state, and have centered upon economic relations. This was the starting point for Christaller himself (1966), who was studying distributional relationships within south Germany, and it has been the perspective of many of his most interesting followers (e.g. Skinner 1964). It begins by assuming things which the archaeologist needs to discover.

Other workers, in the field of urban geography, on the other hand, have often ignored political boundaries altogether or alternatively have considered the frequency distribution of cities of varying size within the given political units, without taking locational factors into account at all. Work of this kind on primate cities (Jefferson 1939; Berry 1961) has nonetheless proved very stimulating to archaeologists and anthropologists (e.g. Smith 1976) and develops ideas more promising for archaeology than those of the sometimes rather mechanistic classical Central Place Theory approach.

Rather curiously, the political geographers themselves have not in general shown the quantitative sophistication of their economic colleagues, despite the useful programmatic statements made, for example by Hartshorne (1950), Jones (1954), Pounds and Ball (1964) and Cohen and Rosenthal (1971), and perhaps most usefully from the anthropological viewpoint, by Soja (1971).

The archaeological problem, at least in the first instance, is to find a means of reconstructing the political organisation, and in particular of deciding which were the dominant centres (see Marcus 1973; Hammond 1974). Often this has been attempted by applying a straightforward Central Place Theory approach, preparing a frequency histogram for sites in terms of their size and looking for 'natural breaks' in the distribution, on the assumption that the sites

may be divided into first, second and third order centres in terms of absolute size (e.g. Johnson 1972, for a pioneering application of Central Place Theory to archaeology). But in a large area it is perfectly possible that centres will actually be much smaller in absolute size than others. They may even be smaller than some of the 'second order' centres of the largest units. This seemingly technical point is significant: it means that a simple frequency distribution approach is not adequate.

It is necessary instead to return to the crucial ideas of power and of *dominance* and to develop an approach which will make clear which centres are dominant and which are subordinate, while allowing for some differences in scale among the territorial units. This idea runs counter to the inverse square law interaction model used by some workers. It has not so far been developed by the political geographers, although it has been illuminatingly discussed by Stinchcombe (1968, 217-31). Some of the most interesting recent archaeological articles dealing with the exercise of power in space (e.g. Steponaitis 1978, Alden 1979) have managed to avoid the pitfalls of too close an adherence to traditional Central Place Theory, but not to the point of rejecting the inverse power or 'gravity' model (cf. Renfrew 1981, 269), as advocated here.

In Chapter 3 a different approach is set out, which avoids this difficulty, although it has to make its own simplifying assumptions. In particular it does assume that the power centre or capital of a given polity will always be the largest centre within it. As indicated in Chapter 3, this is not the case for modern federal states, where Washington D.C. is smaller than New York, for instance. This point has been well brought out by Blanton (1976) with his discussion of the 'disembedded capital'. He rightly indicates that, in addition to the case of federal capital centres, there are other instances where the seat of power within a territory may not in fact be its largest centre. The 'roving palace', a term he aptly applies to the mobile courts of early mediaeval Europe, is one such, and the 'temporary capital', where a new city is built on the accession of a new ruler to the throne, is another (see Willey 1979). These cases do offer difficulties to the model presented here.

The papers which follow, however, are intended as exploratory rather than definitive. They do develop an approach to the archaeological investigation of social organisation in terms of power and of dominance. The first of them offers a very general discussion which leads to the specific procedures, applied in a series of computer-based applications, set out in the second. The underlying concepts

seem to apply to all scales of human societies: the notion of 'polity' as used here does not imply that we are dealing only with urban communities or state societies. Evidently the specific model advanced here has a number of limitations. But I would claim that these papers are a deliberate step towards the development of methods which allow some aspects of the social organisation of hierarchically structured societies to be investigated systematically.

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2

SPACE, TIME AND POLITY

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How can the archaeologist reconstruct the social organisation of prehistoric communities? Can we hope to trace the evolution of societies from the palaeolithic hunting group to the modern state from archaeological data by the use of logically tenable procedures of reasoning, or must any statement about the social organisation of an early society be based upon a facile and ultimately untenable 'analogy' with some entirely unrelated modern community? In this article I hope to approach this problem in terms of spatial organisation – not because the notion of 'territoriality' has recently been fashionable in anthropology, but because the insights of the modern human geographer into spatial organisation offer many potential applications to archaeology which we are only just beginning to explore. In general the geographer, with a frequently simplified model of modern economising man blindly obeying a Law of Least Effort or a Principle of Least Action, is no more concerned with the social organisation of non-market societies than is the structural anthropologist with a Christaller hexagonal lattice. Yet, as I hope to show, the evolution of human society can profitably be considered in terms of spatial patterning. To do so, however, inevitably implies for the archaeologist the final abandonment of the simple notion of 'culture' with its counterpart of 'people' as a fundamental unit of discussion, for these impose upon the data, and hence upon our vision of the past, categories of thought which seem today of doubtful value or validity, obscuring just those questions which we may hope to answer. A further factor contributing to the failure of archaeology to arrive at a coherent view of the development of social organisation may be the sometimes uncritical use of ready-made anthropological concepts which are validated by reference to data different from those of archaeology. It is our task to formulate

concepts and ideas which can operate upon archaeological data, but which do at the same time have real meaning and interest when applied to living societies.

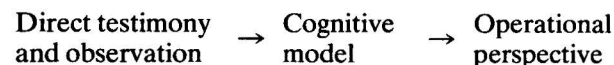
Social Anthropology and Social Archaeology: a Distinction

The prospect of reconstructing prehistoric social organisation has been regarded by some anthropologists with pessimism (Leach 1973). Given the limited range of ethnographic reporting before the accounts of Cook and Forster, the view that 'The belief that something significant about the sociology of a remotely antique human society can be inferred from a study of its material residues is similar to supposing that if we had a random selection of the utterances of unknown meaning from some dozens of unknown and now disused languages we could use this evidence to validate useful general propositions about the nature of human language' (Leach 1974) effectively abandons the hope of studying non-literate societies prior to about 1700 AD.

In order to counter this claim, it is necessary to bring out the rather special interpretation of social organisation which underlies it. Leach is so mesmerised by the variety in human communities, by the intriguing uniqueness of each, that no suspicion of discernible order or pattern emerges: 'in archaeology the accumulation of more and more evidence increases our information about historical artefacts; it does not increase the probability of our sociological guesswork, since the whole range of contemporary ethnographic variety still needs to be taken into account for each new sample of evidence' (Leach 1974). The image which this statement conjures up of social anthropology as a disordered thesaurus of anecdotal observations, entirely lacking in structure, may not be what Leach intends. The dissociation between 'material residues' and 'sociology' does establish, however, a curious polarisation between mind and matter which the best of recent anthropological work, including Leach's own earlier writings, has managed to avoid.

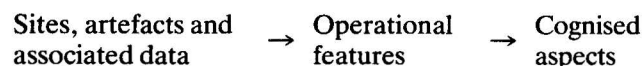
The crux of the matter may well indeed be language, as Leach's analogy perhaps betrays. For the social anthropologist invariably proceeds by learning the language of the community which he is studying, seeking to perceive the world as his informants perceived it, working towards a shared perception, a shared cognition, which he can then interpret. From this cognised model which he forms of the society (and the world) as its members see it, he proceeds to an analysis of the society as it operates 'in reality', that is to say from

the standpoint of the modern anthropological observer. The focus of study shifts as follows:



Whether dealing with kinship, myth or religion, the social anthropologist has traditionally worked in this way, the functionalist school focusing upon the relation between the cognitive and operational aspects within specific societies, the structuralists comparing the cognitive perceptions in different societies. To the outsider it often seems that the social anthropologist is more concerned with what people think than with what they actually do, which perhaps partly explains the comparative neglect into which ethnography has fallen.

The prehistoric archaeologist has no access to direct verbal testimony, and testimony of any kind – in the form of visual representation of the world or other explicit symbols – forms only a small part of his material of study. His progression must therefore be from the data – the artefacts and associated materials in their contexts – as relics of past actions, towards a knowledge of these actions (whether individually or in aggregate) and hence of the operation of certain aspects of the society. In certain cases it may be legitimate to infer or propose aspects of the world picture, the working of the society as recognised by that society, from such operational data:



I suggest that it is a failure adequately to distinguish these very different procedures, to distinguish between the actions of a society as interpreted by its members and as analysed by the modern anthropologist, which has recently darkened counsel. The distinction has been made to very good effect in practice by Rappaport (1969). And it is, of course, precisely that which some anthropologists seek to convey by those infelicitous and synthetic terms 'emic' (i.e. cognitive) and 'etic' (i.e. operational) (Harris 1968a). Harris's words indeed offer the archaeologist comfort and encouragement: 'You are relatively free from the mystifications which arise from the emic approach . . . Your operationally defined categories and processes are superior to the unoperational definitions and categories of much of contemporary cultural anthropology . . . Archaeologists, shrive yourselves of the notion that the units which you seek to reconstruct must match the units in social organisation

which contemporary ethnographers have attempted to tell you exist' (Harris 1968b).

But what then are the effective units of social organisation, these 'operationally defined categories of the archaeologist?' Elman Service, after offering us a hierarchy of band, tribe, chiefdom and state, increasingly used by the archaeologist, dramatically lost his nerve, suggesting instead 'Three aboriginal types which might represent evolutionary stages: (1) the Egalitarian Society out of which grew (2) the Hierarchical Society, which was replaced in only a few instances in the world by the Empire-State that was the basis of the next stage (3) the Archaic Civilisation or Classical Empire' (Service 1971). This offers an alternative so imprecisely formulated that even the initial capitals can do little to recommend it. Are the other existing archaeological categories and procedures for dealing with social organisation as 'superior' as Harris flatteringly suggests?

*The Archaeological Conundrum:
What Is a Social Group?*

For fifty years archaeologists have imagined that they knew the answer to this question. In Europe it was most clearly expressed by Gordon Childe, who introduced the concept of a culture as entity with extension in space and time (Childe 1929). He wrote:

We find certain types of remains – pots, implements, ornaments, burial sites, house forms – constantly recurring together. Such a complex of regularly associated traits we shall term a 'cultural group' or just a 'culture'. We assume that such a complex is the material expression of what today would be called a people.

The same complex may be found with relatively negligible diminutions over a wide area. In such cases of the total and bodily transference of a complete culture from one place to another we think ourselves justified in assuming a 'movement of people'.

Precisely this view, which has been stigmatised in recent years as the 'normative approach' (Binford 1962) has been lucidly reasserted recently by Rouse (1972, 62): 'Prehistorians start with the archaeological facts and from them synthesise a picture of ethnic groups and their distribution, nature and development, using an inductive strategy . . . [There are] four questions that the prehistorian attempts to answer by means of his research: (1) Who produced the archaeological remains under study, that is to which ethnic groups did the producers belong? (2) Where and when did each

group live? (3) What was each group like in its culture, morphology, social structure, or language, as the case may be? (4) How and why did each group become that way?

On this view the archaeologist makes two important implicit assertions, neither of which is explicitly questioned. First that the archaeological record contains homogeneous assemblages of objects (i.e. cultures) which may be distinguished as entities from other assemblages; second that these homogeneous distributions are the material expression of a social reality, the ethnic group. This term, 'ethnic group', is rarely defined with precision, but does not necessarily have racial implications. Rouse (1972, 6) uses the term 'in a general sense to refer collectively' to four different kinds of groups: cultural, morphological (i.e., racial), social, and linguistic: 'A "people" and its culture are but sides of the same coin, the term "people" referring to the individuals who compose the group, and the term "culture" to the activities that distinguish the group.'

Already in 1958, Willey and Phillips (1958) recognised the difficulties in such a view, and admitted that 'phase' (which in their special terminology is analogous to Childe's term 'culture' with its specific and often restricted spatial distribution) cannot be directly equated with 'society'. In their words:

The equivalent of phase, then, ought to be 'society', and in a good many cases it probably is . . . Unfortunately in practice it does not work. We have no means of knowing whether the components we group together into a phase are the same communities an ethnographer . . . would group into a society. We cannot be sure that the individual members of these communities would recognise themselves as belonging to the same 'people' . . . In sum, it looks as though the present chances are against archaeological phases having much if any social reality, but this does not prevent us from maintaining that they can and that in the meantime we may act as if they did have.

The normative approach has been criticised to good effect by Binford (1962) on the appropriate ground that 'change in the total cultural system must be viewed in an adaptive context, both social and environmental, not whimsically viewed as the result of 'influences', 'stimuli', or even 'migrations' between and among geographically defined units'. In a subsequent article, Binford (1965) carries the discussion much further, suggesting that 'culture be viewed as a system composed of subsystems', advocating the separate study of the subsystems within specific culture systems, so as to avoid the holistic simplicities of the normative approach. A distinc-

tion is made between adaptive areas – within which similar techniques for exploiting the environment are used, whatever the stylistic variation – and interaction spheres, large areas within which social interactions occur. The problem of identifying the geographical extent of the specific culture systems to be studied is not considered – indeed the article does not rule out the possibility that this decision might be an arbitrary one. In any case the question of recognising social units is not specifically considered.

Ironically too it is a problem which the increasing taxonomic sophistication of the archaeologist has obscured rather than clarified. For in practice, in specific cases, it has often been difficult to define the boundaries or extent of particular archaeological cultures, and this might have led to doubts about the validity of the concept itself. But instead a willingness to consider at once the differing distributions of the various artefacts which go together to make up the 'recurring assemblage' which defines the 'culture', – a polythetic approach, in other words – serves to overcome the difficulty. David Clarke (1968) discussed the problem in some detail, and finally established a hierarchy of attribute, artefact, assemblage, culture, culture-group and technocomplex. Ultimately however, and with some sophistication, culture as an entity (as defined and established by the archaeologist) is equated by Clarke with social groups as formulated by the ethnographer:

An archaeological cultural assemblage is not identical in space or time distribution with a tribal group, a language, or a sub-race and these sets themselves share different boundaries. Nevertheless, archaeological culture is most likely to have been the product of a group of people with a largely homogeneous tribal organisation, language system and breeding population – whether the people themselves recognised the set or not. The archaeological culture maps a real entity that really existed, marking real interconnection – that this entity is not identical to historical, political, linguistic or racial entities does not make it the less real or important. The archaeological entities reflect realities as important as those recognised by the traditional classifications of other disciplines; the entities in all these fields are equally real, equally arbitrary and simply different.

I suggest that the only solution is the total abandonment of the notion of the culture (and the 'phase' of the Midwestern taxonomic system) as recognisable archaeological units. For very often, while imagining that we are allowing real patterns to emerge from the

archaeological data – patterns which must, it is thought, betoken some social reality – we are instead imposing taxonomic categories upon the data which are purely constructs of our own devising. Of course we are at liberty to classify the material remains of the past in any way we choose, but the inference should be avoided that such arbitrary categories mean anything in terms of ‘peoples’ or ‘societies’, if these themselves are conceived as more than the arbitrary creations of the modern archaeologist.

For it is easy to show how spatial distributions, equivalent to the traditional cultural entities, can be generated by the archaeologist out of a continuum of change. If uniformities and similarities in artefact assemblage are viewed as the result of interactions between individuals, and if such interactions decrease in intensity uniformly with distance, each point will be most like its close neighbours. Consider the point P lying in a uniform plain, with its neighbours fairly regularly spaced around it. Similarity in terms of trait C decreases with distance from P. At the same time the variables A and B vary uniformly across the plain with distance along the axes *x* and *y*. If the excavator first digs at P and recovers its assemblage, he will subsequently learn that adjacent points have a broadly similar assemblage, which he will call ‘the P culture’. Gradually its boundaries will be set up by further research, with the criterion that only those assemblages which attain a given threshold level of similarity with the finds from P qualify for inclusion. So a ‘culture’ is born, centring on P, the type site, whose bounds are entirely arbitrary, depending solely on the threshold level of similarity and the initial, fortuitous choice of P as the point of reference.

If the plain is not homogeneous, but there are in fact barriers to interaction, for instance mountains or water, the rate of change of similarity with distance will be modified, so that similarity clines will emerge. To the archaeologist the real (that is, taxonomically real) entities so observed will serve to confirm his definition of ‘culture’ and its equation with ‘people’ or ‘society’. Of course it does not follow, simply because archaeologists can create arbitrary taxonomic units, that less arbitrary units may not be found. But if the question is firmly posed, it is clear that such entities could only maintain their unity or homogeneity through the persistence of interactions operating preferentially among their members. These interactions would not in general be operating with comparable intensity between one entity and its neighbour. Some of the archaeological units termed ‘culture’ are so large in extent that this intense sharing is questionable. One would wish to be assured that the

differences observed between the archaeological remains found at points P and Q are more than the result simply of diminished interaction resulting from distance or limited accessibility. Ian Hodder at Cambridge has been the first, I believe, to study archaeological distributions from this standpoint (Hodder and Orton 1976), and his approach is clearly a very fruitful one.

The archaeologist’s interest clearly is not – or at least should not be – limited simply to the attenuating effects of distance upon human interactions. In attempting to say something more about human society, his interest must centre upon specially patterned interactions which are indicative of organisation, or the membership of some society or group.

Seen in this rather sceptical perspective, the notion of social group is seen urgently to require definition: it cannot be allowed to emerge from the archaeological data as an uncritical distribution of similar artefacts. And the idea of ‘people’ as deriving from social group begins to look a very doubtful one. One might well enquire, indeed, whether the notion of ‘people’ in the sense of a large group of broadly similar and related individuals and greater in size than individual community groups, is not itself dependent upon the existence in the world of large political units. The notion of ‘one people’, familiar from the modern nation state, can perhaps be traced back to the Romans, but it may not be necessary to take it very much further. Both Binford (1972) and Isaac (1972) have asked at what point in human development did ‘ethnicity’ come into the world. And very possibly social groups beyond the band level, with special interactions within them, may have developed during the palaeolithic. But to take this further, to suggest widespread, homogenous units of distinct and distinguishable ‘peoples’ prior to the emergence of very large political units or ‘empires’ may not be warranted. What is a people if it is not a political unit? The anthropologists who speak of ‘tribes’ may have the answer, but the archaeologist will ask what is the essential unitary nature of the tribe, if it is not a political unit (Fried 1968). The observed ‘culture’ defined by the archaeologist may arise from special interactions at the time in question due to social affiliation. It may even be observable as a result of past interactions within the area, even though that social affiliation has now broken down – this is what some writers mean by ‘tradition’. But often it is no more than a reflection of diminishing interaction with distance.

The moral to be drawn from this discussion is not necessarily a pessimistic one, even if the taxonomic manipulation of artefact

distributions can no longer be expected automatically to yield social or ethnic information. It is rather that to produce social answers we must formulate social questions. When we go ahead and do so the problem emerges in a very different light. We have to ask ourselves what we mean by social organisation – or rather what we choose to mean by it, since the discussion in the first section indicated that our choice may be something very different from that of the social anthropologist, and it is at this point in the discussion that the possibility arises of breaking new ground, with the realisation that the traditional archaeological notion of social organisation may be as invalid as the traditional anthropological one is inappropriate.

The archaeologist may well be concerned to establish many things about early societies which the anthropologist will take entirely for granted. And the first of these could be, indeed should be, to identify units of analysis. The social anthropologist can avoid this problem on arrival to conduct his fieldwork by (a) asking 'Who are you? To what group do you belong?', and (b) requesting 'Take me to your leader'. Few anthropologists or archaeologists have recognised the fundamental nature of the assumptions which these procedures entail.

Is there in fact any area of administrative or 'ethnic' unity, beyond that of the individual settlement unit? What is its size? Does it occupy a specific territory? With boundaries? Is the community sedentary – over one year, over many years? Can it be subdivided into smaller social units? Is settlement dispersed or aggregated? What hierarchies of power, of prestige and of wealth exist within it? Is the administration – political, religious or economic – centralised at all? Do these functions overlap in a single place or person? What investment of labour and produce is devoted to the organisational and religious centres? By what means are status, power, wealth and sanctity expressed or symbolised? What degree of specialisation is there in these fields, as well as in agriculture and technology? What other kinds of interaction take place within it? Is the group affiliated with neighbouring groups by joint membership of any kind of association? Is it related to neighbouring groups by any position of dominance? What interactions take place with neighbouring groups outside the territory?

These indicate the kind of question which the archaeologist will wish to ask – and of course many of the answers would be self-evident to the field worker in a living society. In framing them it was no longer possible to avoid the idea of the group as in some sense an entity – the term is meaningless otherwise. This implies the assump-

tion that, at least among living communities, societies or groups can be recognised that are not merely the product of interactions varying in intensity with distance yet otherwise unstructured. Because human beings are acutely aware of many social relationships I suggest that they will be explicitly aware of membership of such groups – these are therefore cognised entities, and in any given case a specific individual is either a member of a group or he is not. Clearly an individual can belong to several groups, but not without being aware of it. The kind of social group we are speaking of here is relevant to the notion of identity, to the 'Who are you?' question. So that operative groups, visible to the modern observer but not to the participant (e.g., 'middle income group', or group sharing a specific blood type) are not relevant. But for once we are not defeated by the evanescent, irrecoverable nature of past cognitive categories. For it is precisely the recognition of the group that governs group behaviour. When we are dealing with social groups their cognitive existence, recognised by those included and those excluded, reinforces their operational existence or may indeed create it. And the distinctive group behaviour – the differential nature of interactions with members of the group in distinction to those outside it – reinforces its cognitive recognition. It is this feedback mechanism that gives social groups their distinctiveness – for the group does not count as such if you cannot recognise it. (Even secret societies are no exception – considerable care is taken that their members should be mutually recognisable.) This being so, we ought to be able to see them, in favourable cases, in the archaeological record.

Social Organisation and Social Groups

In what I have written so far there has perhaps been a preoccupation with the definition and reality of entities of supposed 'social groups'. The social anthropologist will remark that there is more to social anthropology than this, and may find the foregoing critique of the entities traditionally used by the archaeologist altogether unsurprising. Indeed it could be argued that many of the social aspects studied by the anthropologist could function without the recognition of groups at all.

The whole web of kinship relations, for instance, is often studied in terms of *rules* of behaviour operating within the specific area under discussion. There is no reason here why its bounds need be defined. We can envisage a network of local kin relationships extending indefinitely in all directions, accompanied by a gradual and

fairly continuous change with distance in the rules of exchange. We could then contrast the practices in operation at points A and B without any need to consider the definition of entities.

This brings us right back to a fundamental question which should perhaps have been posed earlier: just what do we mean by 'social organisation' in the context of a study of its evolution? Clearly the term entails consideration of the whole field of human interactions – of languages and symbolic systems, of kinship, or resource allocation between individuals, of government and war, and of other forms of exchange. In each of these fields, entities or groups may be defined: language groups, kin groups, economic units, polities, sodalities, etc. Or in each of them the discussion may well proceed without any definition of such entities.

I would suggest that, in studying the evolution of social organisation, what we are in the broadest sense concerned with is the emergence of inhomogeneity. It is true that in the case of the hypothetical network mentioned above, there can be complete continuity, with one individual acting much like another, and we may nonetheless be concerned with the rules of behaviour, that is to say, with the social organisation of the system. But in a certain sense, however elaborate the rules of interaction may be, the lack of specialisation and differentiation of this network would lead us to regard it as existing at a low level of organisation. When we think of higher levels of organisation we are certainly thinking in terms of *differential interactions* among the individuals participating in the organisation. Of course these interactions can be of many different kinds, so that two individuals may interact strongly in one subsystem of society, as it were, and hardly at all in another. It is precisely when a number of individuals do interact fairly strongly in a number of different ways, and less strongly with others outside that number, that we can begin to speak in terms of a social group. I would suggest that it is difficult to imagine a social hierarchy of any kind without the effective fulfilment of this condition.

It remains to ask just what kind of group the archaeologist is likely to be interested in. And here the material nature of the archaeologist's finds does impose some restrictions. For we can only legitimately speak, as archaeologists, of activities which leave at least some discernible trace in the material record. Prehistoric linguistic groups, for instance, cannot be recovered archaeologically unless there is some kind of correlation between language and the material world. One aspect of organisation which is very pertinent here is administration: it implies both government (with the

exercise of power) and resource allocation. Both imply some partition of the world into persons and resources governed or allocated, and those which fall out of the control of the administration, and hence by their very nature some kind of delineation of an entity, the polity.

An increasing number of archaeologists have come to rely on the 'neoevolutionary' classification of social organisation in order to consider human societies. The hierarchy of band, tribe, chiefdom and state (Service 1962) has recommended itself to many workers concerned to make generalisations of some kind about culture change, or to fit specific societies under study into some more general perspective. The notion of chiefdom in particular has helped to fill the gap in the workings of earlier anthropologists between the tribe and the state.

These classificatory terms have been criticised by some, either because they are not observable from the archaeological record (e.g. Leach 1973) or because they have no real existence (Fried 1968) or again because the hierarchy is thought to imply some evolutionary necessity (Tringham 1974), an implication which those who have used these terms would reject. Like the terms in any classificatory scheme they are constructs which are justified if found useful in practice. And their application to the prehistoric past is greatly strengthened by the effectiveness with which they have been applied to living societies. Certainly they appear less regularly sequential than classifications with a strong chronological component such as Palaeolithic, Neolithic, Bronze Age, etc, or Formative, Classic, Florescent, Decadent, etc. They certainly supersede the older social categories of savagery, barbarism and civilisation, which later took on both chronological and economic/technological overtones in a confusing way. Childe sometimes used barbarism as the equivalent of Neolithic-to-Bronze Age where today we find it convenient to speak of hunter/gatherer bands and agricultural tribes.

The real operational difficulty of recognising these forms of society in many cases should not be underestimated, however – which is not the same as Leach's assertion that to do so is logically not possible. And it should not be overlooked that increasingly they are being distinguished in terms of settlement pattern. For the sake of argument, and to stimulate discussion, I would like to suggest here that tighter and more effective definitions can be obtained by replacing the band/tribe/chiefdom/state hierarchy *in toto* with a classification based on the spatial arrangement of society.

The term band, for instance, is recognisable by its small size – normally less than 100 persons – and by the impermanence of its place of residence. (For some reason no one calls a sedentary village of the same number of persons, living in complete isolation, a band – it is automatically considered ‘tribal’, generally without the application of any positive criteria to justify that term.)

And at the other end of the spectrum the term state has always proved almost impossible to define – just as difficult indeed as ‘city’ and ‘civilisation’, with which it generally overlaps. At a recent conference in Santa Fe it proved very difficult to hit on any criteria which can be recognised archaeologically, since the one clear criterion proposed by Service is not a practicable one, ‘the presence of that special form of control, the consistent threat of force by a body of persons legitimately constituted to use it’ (Service 1962, 171). Instead both Wright (1978) and Johnson (1975) suggested that the most characteristic feature was a three-tier hierarchy of control, which is best seen archaeologically in the trimodal settlement size distribution of city, town and village.

Turning now to chiefdoms, I suggest that their most distinctive feature in the archaeological records is the presence of central places. For the central person who is the permanent chief is generally situated at a central place, even if this may be a periodic one, either in the sense of rotating (i.e., changing regularly in location) or in the sense of operating only for part of the year. Most of the Polynesian chiefdoms were of the latter kind. But the central place was usually dignified by special buildings pertaining to the chief, and sometimes by monumental ones relating to ceremonies of life or death. As Service remarks, chiefdoms are distinguished from tribal societies ‘by the presence of centres which co-ordinate economic, social and religious activities’ (Service 1962, 173).

One cannot go so far as to assert that those societies which are not organised as chiefdoms or states, or with comparable complexity, entirely lack central places. But it is difficult to point to permanent central places with a permanent population which do not, in fact, exercise a central administrative authority over the territory which they serve.

As for the tribe, the concept remains archaeologically undefined. Steward (1955) has stigmatised ‘tribal society’ as an ‘ill-defined catchall’, and Service’s definition is perhaps the least satisfactory of any in his hierarchy. It gives little clue as to how the limits of the tribe in space might be ascertained: ‘Pan-tribal sodalities make a tribe a tribe, for if they did not exist then there is nothing but a series

of bands, more affluent than hunters and gatherers, but still bands, with only intermarriage between certain ones providing any unity’ (Service 1962, 115). Nowhere do we find a clear picture of the nature of those interactions, taking place within the tribe but not across its boundaries, which work to maintain and enhance its unity and homogeneity. Until we have some inkling of what we are searching for in the archaeological record we shall not find it, for, as I have indicated above, the simple fall-off in interaction with distance is sufficient to allow the generation of ‘cultures’ by the archaeological taxonomist which need have no cognitive meaning.

Fried’s criticisms of the concept of tribe have not been adequately answered, and his suggestion that ‘most tribes seem to be secondary phenomena . . . the product of processes stimulated by the appearances of relatively highly organised societies amidst other societies which are organised much more simply’ (Fried 1968, 15) is paralleled by the suggestion above that the notion of ‘a people’ is largely the product of modern (or early imperial) nationalist thought.

Goody’s discussion in *The Social Organisation of the LoWiili* (Goody 1967) is particularly useful here, for he makes it clear that the recognition of tribal entities among the Dagari-speaking peoples is largely the act of outside observers taking relative terms as fixed, tribal designations: the diffuseness of the political organisation gives rise to the lack of distinct tribal nomenclature. Such designations as exist, however, arise not primarily from a consciousness of unity, which would surely lead to a recognised name within the tribe itself, but ‘from a consciousness of the differences between their own institutions and those of neighbouring peoples’ (Goody 1967, 113). Goody shows clearly (chapter 5) that the basic territorial division of the land in the region is the ‘parish’, the ritual area associated with a particular earth shrine. The inhabitants of this area may be described as a political group, and it is clear that in any discussion of social organisation the ‘parish’ must be a fundamental unit of discussion, although the members of several parishes can meet at a market for purposes of exchange. The case of the LoWiili is a particularly interesting one for the archaeologist, illustrating as it does the diffuse nature of a supposed social grouping above that of the operative and cognised territorial units. The ‘tribe’ is a dangerous concept for the archaeologist.

This critique is not offered in a negative sense however, since the ‘evolutionary’ hierarchy at least gives us something to compensate for the failure of archaeology to talk about social organisation at all until very recently. A discussion in spatial terms is operationally

more convenient and perhaps no less apposite for social description. For is not the most important feature of a chiefdom the existence of a central person, resident at a central place, whether this be periodic or permanent? And is not the distinguishing feature of the state generally accepted as the existence of a permanent hierarchical structure of administration and authority – a structure generally reflected in a hierarchy of central places, so that between the major centre (or state capital) and the minor residence unit (or village) there is at least one intermediate settlement and administrative unit, the regional centre?

Polity and Space

The spatial structure of society has never been adequately explored. Locational analysts have developed sophisticated techniques in modern geography for investigating settlement distributions, but their implications in terms of social organisation have not been investigated. Anthropologists have written of territoriality, but focused upon the behavioural analogy between non-human primate behaviour and the most closely analogous human societies, generally choosing nonsedentary societies. And thinkers such as Eliade (1965) have commented on the symbolic aspects of space without, however, using it in the study of society as a whole.

I suggest that there is a whole range of generalisations to be made, many of them exceedingly obvious (and no doubt formulated in isolation often enough) which together might lay the foundations for a general study of human social organisation in a manner that is operationally explicit, so that it can, in favourable cases, be applied to past societies as well as to living ones. Some of these possibilities have been indicated in a number of useful articles by Trigger (1972 and 1974).

The presentation offered here, like the whole of this paper, is provisional. But I hope it may serve as a basis for discussion.

(1) *The basic social group is defined by the habitual association of persons within a territory.* These are the persons who live together throughout the year, what Murdock terms the community, 'the maximal group of persons who normally reside together in face-to-face association' (Murdock 1949). Among hunters this may be a group of families who normally camp together, i.e., a 'band'. Among sedentary communities with fairly aggregate settlement it will be a village, or in some areas such as south Italy without villages, a town. With a dispersed settlement pattern it may even be a household. It is often possible to subdivide the group into families

or households, but these live at the same location: if they split up for part of the year the smaller units so formed would be regarded as the basic social group.

This definition implies two statements about human behaviour. First that there are such habitual associations. And second that human behaviour is territorially organised. This operational assertion carries with it cognitive implications: 'L'installation dans un territoire équivaut à la fondation d'un monde' (Eliade 1965, 43).

Perhaps the most fundamental assumption of all, however, is that the individual can belong to only one such basic social group at a time, whatever his affiliations to groups of other kinds.

(2) *Human social organisation is segmentary in nature: human spatial organisation is therefore cellular and modular.* This is to say that the basic social group, with its territory, is repeated in space, so that a simple cellular pattern is generated. The boundaries of the units need not, however, be particularly well defined: the territory may rather consist of a 'core area' with a 'development of a territorial gradient from the 'core' to the 'periphery' of their home range' (Flannery 1972a). What is being asserted here, however, is that the essential unit, the basic group, whatever its precise nature, is repeated spatially. There is, of course, no *a priori* reason why this should be so – an entire range of different kinds of such basic groups could live in juxtaposition, each quite unlike the others in its scale or internal structure. But this is not the way it works in practice. The adjacent units do have comparable organisation and also comparable size – which is the implication of the term modular.

This modular feature of the segments, that in a given region the territories are of approximately the same size is a feature not generally discussed, but appears in fact to be one of the most fundamental features of human social and spatial organisation. It has been demonstrated for the Australian Aborigines by Birdsell (1973) who has shown that the modular size of 'hordes' in Australia correlated with rainfall in a regular way.

Binford (personal communication) is at present working on more general regularities in the territorial group sizes of hunters and gatherers. But clearly the concept bears investigation in more general terms, since a modular arrangement is evident among sedentary village societies likewise, and the idea is inherent in most Central Place studies, although rarely brought out explicitly.

Archaeologically the important aspect of this observation is that it can in fact be investigated. Studies of settlement spacing of basic groups have in fact been undertaken (e.g. MacNeish 1964), al-

though clearly there is a risk of error when the settlements considered are not precisely contemporary (cf. Ellison and Harriss 1972).

Birdsell's work promotes the thought that we may be dealing not primarily with a territorial module but a population one – that is to say that it is the size of population of the groups which remains stable rather than the area they occupy. The two will be the same only when ecological conditions are constant. There may be underlying social reasons for such modular size, that is to say thresholds beyond which the group population cannot grow without a change in the nature and structure of the group (cf. Forge 1972).

Alternatively there may be mechanisms which tend to keep the territories and population sizes of groups in a given region at about the same size one-to-another, without discriminating in favour of a particular size range. Rowlands (1972) has discussed the relation between territoriality and defence, and one can envisage factors which might work toward parity among the social units (and hence for a *local* modular size).

Archaeologically this pattern is a very frequently recurring one, discernible in almost every case where there are sedentary communities. When the settlement is aggregated into villages, their spacing and placing can be viewed in these terms. Such a patterning may certainly be discerned, for example, over much of prehistoric Europe. The notion of modular organisation, for what has been termed the Early State Module (ESM) has been discussed further in a recent paper (Renfrew 1975; see pp.94–101 below).

(3) *Basic social groups do not exist in isolation, but affiliate into larger groups, meeting together at periodic intervals.* The affiliated groups are generally adjacent to each other, so that their territories, taken together, may be regarded as forming a larger, continuous and uninterrupted territory.

Among hunting groups the meeting place itself may vary from time to time (Birdsell 1973). Among sedentary communities the place of meeting may move in rotation (cf. the Kyaka). Often it is a permanent meeting place, however – that is to say the meeting always takes place at the same location, and it must then rank as a central place. (If the meeting takes place only on specific occasions through the year it can be regarded as a periodic central place.) Moreover it is a feature of many societies that they express the uniqueness of this location by symbolic means, sometimes by monumental construction, since the location may have a religious significance as well as expressing the affiliation of groups itself. Once

again the observed operational reality has further cognitive overtones: 'Notre monde se situe toujours autour d'un centre' (Eliade 1965).

This is precisely the situation described in detail for the LoWiili by Goody (1967, chapter 5 and fig.6), where the Earth Shrine is the focal point of the polity, and the principal feature of its territory. In archaeologically favourable cases these constructions are of a durable nature – in the Tuamotu islands of Polynesia the *marae* is the focal point of the lineage territory (Ottino 1967), and archaeological research has produced patterns in location (Garanger 1966) which even without the ethnographic observations available would have been suggestive of a cellular and modular territorial arrangement and social partitioning (see pp.178–80 below).

Analogous spatial patterning has been noted among the collective tombs of Neolithic northern Europe (see chapter 8 of this book), and a segmentary social grouping suggested in consequence. The monuments are there viewed as the traditional ceremonial focal point in a territory where household location is either dispersed or shifting every few years. The modular aspect of the patterning is reflected in the approximately equal size of territories, or more often in the approximately equal area of arable land within each, suggesting approximate equality of population. In some cases, as Fleming has pointed out (Fleming 1972) the interior division of the tombs is itself cellular, reflecting perhaps the division of the territory into several households, or other sub-division, each of which would have access to one part of the tomb.

(4) *Human society is hierarchical in nature: human spatial organisation is therefore stratified.* This generalisation effectively follows from the cellular pattern discussed above. Once the cells are contiguous, sharing common boundaries, they can no longer all expand territorially simply by increasing in size. It would, of course, be possible for some to expand at the expense of others, so that there would be fewer, larger cells, each organised just as before. And no doubt examples can be found of such aggrandisement without further organisation or complexity.

Instead, however development occurs by the union of a number of basic cells or segments into the kind of affiliation indicated above with the emergence of a new and (in the sense of our stratification) higher organisation to deal with the consequences of union, *while retaining elements of the former cellular pattern.* These observations hold whether the union occurs by direct conquest, in which case the conqueror takes over the central administrative functions of the

conquered group, or by peaceful assimilation or symbiosis. In each case the union of a number of cells into a higher order cell implies the emergence of a higher order centre, but the lower order centres normally survive.

And growth continues in this way in stratified order, the cellular units at each level of organisation together forming the sub-units for the next with the emergence of a higher order centre, usually located at the traditional centre of one of the constituent subunits.

Already this process is seen in operation in embryo among the Tiv (Bohannon 1954), an example worth citing because it has been described in explicitly spatial terms. Evidently the process of urbanisation, although it is conventionally considered primarily in terms of the urban centre itself, can perhaps be seen as appropriately of territorial amalgamation of this kind, although the importance of increasing population density should not be overlooked in any discussion of territorial size.

(5) *The effective polity, the highest order social unit, may be identified by the scale and distribution of central places.* In the stratified ordering of cellular territories, the individual person has allegiance at a number of levels – to the basic group (the community), to the logical region focused on its main town, and to the higher order realm comprised of several regions, focused upon its capital. He is a member of each, but he is a *citizen* of the highest order polity. For it is the central organisation of this polity, usually its central person, who wields supreme power. This is the highest level of society, in M. A. Smith's words: 'A group of people acknowledging a single system of law, and in some degree organised to resist attack from other such societies' (Smith 1955). This is the highest social unit effective in political terms, which means in military terms.

Now in order to recognise such a unit archaeologically, one approach is certainly to study the symbolism which the central authority employs to express its power. For instance the extent of the Roman Empire, if it were not known to archaeologists, could be established by plotting the distribution of monumental Imperial inscriptions. This is certainly more effective than trying to follow boundaries (which may not always be delineated by a Hadrian's Wall) or attempting to follow artefact uniformities. For we saw in the last section how archaeologists can create 'cultures' where no social group need have existed. And equally the boundaries of existing social groups may be blurred by interactions across them.

A feasible alternative is to study the hierarchy of central places, establishing first the approximate size of the highest order centres.

Assuming these can safely be identified as such (and here the symbolic evidence may be required as corroboration) their distribution marks the centres of the highest order cellular territories, that is of the polities. And some attempt can be made at reconstructing the subsidiary cells of this stratified arrangement.

Precisely such a study has been undertaken by Johnson in Mesopotamia, with this underlying implication that spatial organisation is the counterpart of social organisation. Spatial hierarchy has been examined in Britain by Hodder and Hassall (1971), although the social implications were less obvious since the centre of the polity was by then outside the British Isles, in Rome. It was clearly brought out in the Tehuacan valley by MacNeish in a pioneering study (MacNeish 1964), and in the Maya lowlands by Flannery, Hammond and Marcus (1972b, 1972 and 1973). The existence of a social hierarchy in Neolithic south Britain has been postulated on the basis of a hierarchy of places (see p.236 below).

The modular arrangement is apparently reproduced at each level in the hierarchy, including the highest, so that for each polity, with its centre (or 'capital'), there will be an analogous one with a contiguous territorial boundary, until the polity is so large that it occupies all the available land of the land mass (i.e. continent) or island group where it is situated.

In asserting the widespread nature of the modular arrangement it is not of course claimed that all the neighbours of a polity need be unified into territories of a comparably higher order – this would be to assert the worldwide nature of a specific social organisation at a given time. But this is, nonetheless, a general tendency over fairly large regions.

The social and political aspects of spatial hierarchy are frequently overlooked by geographers, but it does seem broadly true that the polities of the world each boast a capital city, and that within broad regions these capitals could be recognised by their size and relation to subsidiary centres, as well as by the attendant symbolism, without prior information about national boundaries. (Artificial capitals such as Canberra or Washington D.C. clearly present a specific problem, foreshadowed by the summer palaces of early emperors, but again it is a problem which a consideration of symbolism would help to solve.)

(6) *Special interactions between polities undoubtedly take place, creating uniformities in artefact distribution, and require closer study: such uniformities in themselves do not document societies or 'peoples'.* This is a re-statement of the critique of the normative

approach discussed above. Generalisation 3 (above) contains the germ of an idea, however, that the highest units of the social hierarchy (i.e., the polities) sometimes enter some kind of affiliation without losing any element of their autonomy. This may have been the case in the early Greek city leagues, (Fustel de Coulanges 1873) and at a lower level may be true of tribes which are recognised as such without sharing any central tribal organisation whatever.

Clearly the tribe still presents a problem, which the archaeologist alone cannot be expected to solve if the anthropologist remains in doubt about precisely what a tribe is. I suggest, however, that the concrete approach outlined here, of considering first effective polities and territories, together with their hierarchical arrangement socially and spatially, may be useful, since it is an approach which has meaning both for the archaeologist and the anthropologist. This will leave for further examination the nature of the 'tribal' links across and between polities and territories – the sodalities and other affiliations and the linguistic affinities. It may be that in some cases the symbolism used to express such affiliations may offer the hope of archaeological interpretation.

The procedure adopted here has been to risk certain generalisations about the spatial behaviour of social groups – that is to say as groups recognised as such by their participant members, and recognised as operational by external observers. The consideration has been limited to groups whose members are spatially contiguous, so that the group can be delimited spatially. Clearly other kinds of group could be considered, but they are less easy to approach archaeologically, and in any case it is suggested that polities are indeed bounded units of this kind. If these generalisations can be accepted as holding – broad as they are – for human societies as a whole, then they do offer the possibility for the archaeologist of proceeding from observations of spatial organisation to conclusions about social organisation.

No doubt these specific formulations can be questioned on a number of grounds. But unless some such formulations as these can be made, I see little hope of using archaeological data to suggest conclusions which would interest the anthropologist. Of course it may be argued that the kind of conclusions suggested here are altogether unremarkable, and the generalisations attempted above highly banal. Nonetheless, if they are accepted, they can lead to a clear and simple picture of the growth of some aspects of social organisation, a picture which so far has been outlined only in a few specific areas of study, especially Mesoamerica and Mesopotamia.

Nor should the practical difficulties be overlooked that are inherent in the eliciting of spatial patterning from archaeological data. Incomplete survey evidence, and the difficulties of setting up a chronology sufficiently exact to identify settlements occupied contemporaneously present real problems. And some of the necessary procedures have hardly yet been worked out: there is so far no general treatment of the problems involved in the ordering of places into a hierarchy from archaeological evidence. This may be effected by a consideration of size (Johnson 1972), of labour invested in their construction, of functional aspects such as defences (e.g. Romano-British walled towns; Hodder and Hassall 1972), of monumental ceremonial centres (e.g. Maya centres; Marcus 1973), of the finding of artefacts interpreted as symbolically relevant (e.g. clay nails in Mesopotamia; Wright 1972), and no doubt in other ways. [1974]

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